CONCERNING S. P. KHROMOV'S ARTICLE ENTITLED "REVIEW OF THE BASIC CONCEPTS OF SYNOPTIC ANALYSIS"

G. D. Zubyan

/Note: The following report appeared in Meteorologiya i Gidrologiya, No. 2 (October 1950), pages 27-33, as an answer to another article in the same issue, pages 20-26./

Soviet investigators have contrubuted considerably by their work to the theory and practice of analysis of tropospheric fronts. We find in their work versatile research on problems of kinematics and dynamics, displacements and deformations, space structure and cloud system of frontal separations, problems of frontogenesis and frontolysis, peculiarities of fronts and front activity in physico-geographical circumstances in various regions of our territory.

A progressive step in the theory and practice of analysis of frontal separations is achieved by the work by Kh. P. Pogosyan and the late N. L. Taborovskiy, published in 1948 and entitled "Advective-Dynamical Fundamentals of frontological analysis" (appearing in Trudy TsIP, as issue No. 7 (1948)). The work is a logical conclusion of previous research by the mentioned authors and first of all of investigations of cyclo- and anticyclogenesis and of general circulation of the atmosphere. The work contains many new principles which cannot pass unnoticed in practical work and in further research. Therefore we cannot agree with the basic statements of S. P. Khromov's article, published in this issue; first of all we have to make the following remarks:

While announcing at the beginning of his article that the large size of Pogosyan's and Taborovskiy's work compels him to linger only on basic problems of importance to practical analysis, Khromov really acts differently. He is

busy discussing not the basic, but mostly secondary problems; he diverts the reader from the most important results obtained by Pogosyan Taborovskiy and therefore confuses the reader's orientation. It is probably done with the intention of showing that the work does not contain anything new; but "detached conclusions and assumptions" (e.g. the distinction of warm and cold masses on topographic maps, the criticism of some assumptions connected with occlusion etc) are correct and worth consideration in practical work. How should we interpret otherwise that lost in Khromov's field of view were such important problems of synoptical meteorology the subject of investigation in the work by Pogosyan and Taborovskiy, as kinematics and dynamics of frontogenesis in the troposphere, frontogenesis and dynamic variations of pressure, problems of analysis and forecasting of activity of frontal separations connected to the structure of the thermobaric field, the frontogenesis and transformation of altitudinal deforming fields etc.

Can we seriously consider that problems of terminology, e.g., how to name an aerial mass - polar or temperate, - problems of geographical classification of aerial masses and fronts, the distinction of warm and cold aerial masses on maps of relative baric topography etc, are more important and more pressing than those mentioned above? Certainly not.

In his article Khromov mentions several times the synthesis, the survey of connections between frontological conceptions and "ideas ... of advective dynamical analysis".

From our point of view, works on advective dynamical analysis, in particular the discussed work, do not consider the problem of synthesis or "survey of connections" with previous assumptions, but a new and more correct approach to analysis and to forecasting of atmospheric processes.

The subject of this new approach, of this new method, is first of all the advective dynamical theory of cyclo- and anticyclogenesis. From the point of view of this theory most processes of formation and development of cloudiness and of precipitations are connected with frontal separations. With respect to cyclo- and anticyclogenesis fronts possess significance as zones of larger temperature contrasts, on which (contrasts) dynamic variations of pressure directly depend.

The basis of the frontological method is the undulatory theory of cyclogenesis. The backers of this theory, including Khromov, consider cyclogenesis as a process of generation and development of undulatory disturbance on the front, while, according to the advective dynamic theory, waves on fronts are only kinematic consequences of dynamic pressure variations and formation of depression.

By this clarification it should be plain what Pogosyan and Taborovskiy mean, when they write: "A number of investigations devoted to problems of atmospheric pressure variations and to the genesis of processes, completed by means of maps of baric topography, introduced here in 1937, proved that we may discuss the development of processes on basis of analysis of thermobaric fields of the troposphere, without directly introducing fronts. This is based on the fact that the effect of fronts on dynamics of the process is seen in general in the horizontal gradients of temperature and pressure in the troposphere. Therefore the single front drawn near the terrestrial surface may not supply indications with respect to further development of processes".

By quoting only a part of this phrase without repeating in full what the authors meant, Khromov speaks about the return to isobaric synoptics, on the non-acknowledgement of the reality of fronts, with the purpose, while being unable to produce some more serious proofs, to scare the synopticians.

Nobody doubts and cannot doubt the reality of frontal separations; they represent an objective reality. And the work discussed by us does not intend to deny the actuality of frontal separations, but, on the contrary, looks for new more perfect methods of their analysis. Pogosyan and Taborovskiy and their followers do not deny the reality of existence of frontal separations but the conformity of reality of a number of situations of frontological analysis. This is quite a different matter, and Khromov and others quite uselessly try to convince us to the contrary. The above-mentioned quotation outlines and emphasizes the unequivocal situation that great masses of atmosphere take part in the dynamics of tropospheric processes and therefore their analysis and forecasting could have better success, if based on analysis of thermal and baric fields of the troposphere, and not on "the single front, drawn near the terrestrial surface". It is evident that this is not "a declaration of a possibility to menage without fronts".

We cannot help paying attention to what is said in the end of the third paragraph of Khromov's article. We read there: "The introduction into synoptics of a third dimension may really give fruitful results, namely on basis of such solid frontological concepts as a front and a frontal cyclogenesis, and we should enjoy it that, judging by the work of N. A. Taborovskiy in 1947 and by its review, the authors understood it".

Pogosyan and Taborovskiy never considered frontal cyclogenesis as a solid concept. On the contrary, they proved its inconsistency and worked out a new advective dynamic theory of cyclo- and anticyclogenesis. This is one of their basic merits in synoptic meteorology. Khromov does not want to acknowledge it; he mentions only "concepts" of advective dynamical analysis.

But independent of the wishful thinking individual people, the theory of cyclo- and anticyclogenesis by Pogosyan and Taborovskiy proved practically its vitality and its advantage as a more advanced and complete theory, than the undulatory one. The theory of cyclo- and anticyclogenesis became the basis of much fruitful research. A predominant number of Soviet synopticians nowadays adhere to this theory.

We think it would be more appropriate to apply just to Khromov his own words: "Is it worthwhile to retrograde synoptics?".

Besides, Khromov in his article repeats several times the concept that allegedly: "the advective dynamical analysis rested until 1947 on concepts of advection and vergence of isohyps, which did not reveal the dynamical mechanism of the phenomena". And this is clearly presented, strangely enough, as an argument against the advective dynamical analysis, and in defense of the frontological method. Khromov's statement is entirely incorrect. We do not consider it necessary to return once more to this question, and we refer the reader to our article. (cf: "Meteorologiya i Gidrologiya" No. 3 (1949). Here we intend to draw attention to something else.

Let us assume that Khromov was actually right that: "until 1947 the advective dynamical analysis rested on concepts of advection and vergence, without revealing the dynamical mechanism of the phenomena". Well, what does it mean? This was up to 1947, while nowadays it is no more so, which the author of the referred article does not deny. Can such a statement constitute any argument against the advective dynamical analysis?

In his tendency to deny the basic value of the whole collection of works on advective dynamical analysis as a new stage in the development of synoptic meteoro-

logy, in the tendency to refer everything new developed by the authors to simple "concepts" and therefore to back the insecure positions of frontal cyclogenesis, - Khromov operates by such "arguments".

By reading Khromov's article, people little experienced in synoptic meteorology and not familiar with its present state may be led to entirely false conclusion: what are Pogosyan Taborovskiy worrying about? Why, synoptic meteorology attained perfection, due to fundamentals of the old frontological analysis with its geographical classification of aerial masses and fronts and with its undulatory theory of cyclogenesis; and if some difficulties are involved in practical work, the synopticians are unable to overcome them only because of lack of sufficient experience. Is it so?

The problems of analysis and forecasting of atmospheric processes and of the weather, including the analysis of fronts, require, despite successes achieved at first by the works of Soviet writers, further many-sided, persistent and profound research. Only in this way shall we be able to solve the economibal problem, posed before us, consisting in radically improving weather forecasting. This is the path successfully followed by Soviet synopticians, and all possibilities are supplied to them along this line. Evidently the inexperience of the synopticians is not involved, but the theoretical and practical weak points of obsolete concepts.

It is just from this point of view that we discuss the work by Pogosyan and Taborovskiy. In this work fronts are investigated from new positions and methods to improve the analysis, and the forecasting of really existing tropospheric frontal separations are marked. In such posing of the problem we see the progressive value of the specified work. It is possible and useful to submit to scientific criticism some position established by the authors. It is only

advantageous for the subject. But it is hardly possible to deny its value for analysis and forecasting in order to back obsolete concepts, as it is done by Khromov.

By investigating the frontogenesis and the frontolysis in altitudinal deforming fields, Pogosyan and Taborovskiy establish important states, revealing internal connections of frontogenesis and frontolysis in various parts of the altitudinal frontal zone. On the other side, they come to the conclusion that from the point of view of dynamical reaction on the evolution of atmospheric processes "it is necessary to distinguish two types of frontogenesis: the basic frontogenesis in the troposphere in general and the frontogenesis in the near-ground friction layer", the so called near-ground frontogenesis, which is conditioned by the convergence of friction and appears only at near-ground levels. Spatial analysis of frontal separations shows that indeed fronts exist, the formation of which is due nearly exclusively to convergence of the ground level wind and which exist only at ground level. In variance from tropospheric fronts, in which formation aerial masses of considerable depth of the troposphere take part, these fronts have no essential effect on the development of processes. The separation of fronts on tropospherical and on ground level ones helps the synoptician in correct orientation in the analysis.

In his article Khromov first tries to deny the necessity of front separation on tropospherical and ground level; later he says there is nothing new in it, and a little further he says "... we see a sufficient analogy between the main fronts and tropospheric fronts by the authors, and between the secondary fronts and the ground level fronts by the authors". It is rather difficult to under-

stand Khromov. In any case, we should remark that ground level and secondary fronts are not identical, already because the latter may occur not only on ground level, but also in the troposphere, although weakly indicated in the thermal field of the troposphere. The main and the tropospherical fronts are not identical neither, because not all tropospherical fronts are the main ones. This is the essential and new matter in the separation of fronts at tropospherical and ground level.

Another consequence of the investigation of the kinematics of frontogenesis in the troposphere is the establishment by Pogosyan and Taborovskiy of concepts of individual and of local frontogenesis. The first is defined as frontogenesis of the shifting air volume, the second as frontogenesis in a specified location. The significance of these concepts becomes obvious when the authors pass to their outline of investigation results of dynamics of the tropospherical frontogenesis. But before we treat this subject it is necessary to make the following remark:

Pogosyan and Taborovskiy write in their work that "the observed structure of thermobaric field of access and delta are such, that in the access always an individual frontogenesis exists, and in the delta an individual frontolysis." Such a definition assumes that in the whole frontal zone the angles between isohyps and isotherms are not below 45°, which is not always the case; i.e. these angles are sometimes in the access as well as in the delta larger than 45°. Therefore we should not deny the possibility of an individual frontolysis in the access of the frontal zone and of frontogenesis in the delta. The word "always" in the quoted definition should be replaced by the word "usually".

Studying the problems of dynamixs of the tropospherical frontogenesis the authors of "Advective Dynamical Fundamentals" deduce remarkable conclusions, on which Khromov prefers to keep silent. These conclusions are the following:

1) There exists a well defined connection between individual frontogenesis (frontolysis) and dynamical pressure variations (Footnote: This connection is established by Pogosyan and Taborovskiy for a particular case, when dynamic pressure variations are discussed in relation to convergence and divergence of isohyps only. This connection in a general case is obtained by I. P. Vetlov.) (Individual frontogenesis is accompanied by dynamical pressure increase; individual frontolysis by dynamical pressure drop).

2) Depending on the type of local frontogenesis, one can obtain a sufficiently complete representation of acceleration of dynamical pressure variation (in the case of local frontogenesis the dynamical pressure variation increase; in the case of local frontolysis they decrease).

The theoretical and practical value of these statements for the analysis and the forecasting of atmospheric processes is so evident, that it is useless to discuss them. These statements are successfully applied in the practical procedure of the Central Forecasting Institute.

In their work Pogosyan and Taborovskiy return again to the problem of the origin and transformation of altitudinal deforming fields; they discuss them in connection with tropospherical frontogenesis based on statements obtained by them as results of investigation of kine,atics and dynamics of this process, which is a basically new approach. Conclusions deduced in this problem widen previous representations, deepen them, and are valuable for wider analysis and improve the possibilities of forecasting the formation and transformation of altitudinal deforming fields. Special attention whould be paid to this part of work.

It is not enough to talk about three-dimensional front analysis; it is necessary to find means for its realization. To this topic, an essential part of "Advective dynamical fundamentals" is devoted.

The basis of actually realizing the three-dimensional analysis of front separation is seen by Pogosyan and Taborovskiy in "inseparable connection of structure of motion and activity of fronts with a thermobaric troposphere field". From this point of view, also essentially new, they proceed in their investigations. On one side they prove the fertility and practical accessibility of their method, and on the other side they lead to a number of important conclusions with respect to connections of fronts with the structure of the thermobaric troposphere field IN general, and with the altitudinal frontal zone in particular, with respect to accentuation and blurring, to dynamical significance, spatial structure of frontal separations etc.

It is fully evident that these investigations are not exhaustive. Much work remains to be done in this respect, in particular in the study of formation processes and in the development of frontal cloudiness and precipitations. Besides, some separate conclusions could be made more precise by the authors; e.g., the problem of the structure of the warm occlusion front. Investigations show that although these fronts have in most cases during the winter a structure represented by the scheme of Pogosyan and Taborovskiy, nevertheless occlusion fronts with a warm front of great vertical extent are not excluded. Also the problem of the criterion of temperature contrast requires more precision. But this is not essential. The most important matter for us is that the discussed article marks basically new, from our point of view, and correct methods of analysis and forecasting of frontal separations and its activities, and an outlook for its further research.

Still a few words on "bent" occlusion fronts, having much importance for the front analysis and the comprehension of the regeneration process of cyclones.

Research by the writer of this article (cf: "Meteorologiya i Gidrologiya, No. 1 (1950)) shows that the statement by Pogosyan and Taborovskiy on the impossibility of the existence of "bent" occlusion front corresponds to truth. The wrong concept on the reality of "bent" occlusions is the result of one-sided interpretation based on exceptional analysis of kinematic conditions and front lines on the ground-level synoptic map.

In his article Khromov left much room to the problem of geographical classification of aerial masses and fronts. He first writes that "the geographical classification of masses actually has no direct forecasting value", but later on he writes: "nevertheless geographical classification is rather essential for analysis, and therefore indirectly for forecasting. The geographical type of mass, correctly determined, supplies us with a natural complex of weather conditions, within the range of condensed characteristics of its system (for a specified time and location)". These two statements are essentially contradictory.

We do not intend to discuss whether geographical classification of aerial masses and fronts is necessary to climatologists or not, but it cannot serve as a pillar, even indirectly, for the analysis and forecasting of weather and for the investigation of atmospheric processes. This is corroborated by long years of experience of practical and research work. We also should keep in mind that not "condensed characteristics" and not "natural complex" are required from a synoptician, but a concrete forecasting, restrained to a narrow range and containing information on all weather elements. Such forecasting can be completed

only only on the basis of many-sided physical analysis of atmospheric processes, deriving from distribution and interaction of meteorological elements in the specified time period and in the marked region.

If we do not disguise ourselves by dialectrics, but try to understand it, we hardly have to prove the necessity of geographical classification of aerial masses and fronts. Then it will be useless to menace with "nihilistic chaos", but it will be necessary to acknowledge the correctness of the remark that "concepts of arctic and polar fronts, taken as basis of an analysis of a synoptic map, as a rule lead to difficulties, which could be aboided only by the formalization of frontological analysis. It proved necessary not only to count with real processes, but to squeeze them into frames defined by accepted terminology of fronts and classification of aerial masses".

We cannot blame Pogosyan and Taborovskiy in their illogicalness in this problem. By denying, and quite correctly, the theoretical and practical necessity of geographical classification of aerial masses, they offered in their work instead of one classification another one. Later they nevertheless acknowledged their error, by agreeing to consider it a kind of scientific liberalism, which we consider useful to mention here.

In concluding our article we quote an excerpt from the work by Pogosyan and Taborovskiy:

"The main deficiency of the method of frontological analysis is just that matter which the Norwegian school considered its rational basis, namely: to place foremost the kinematic characteristics of processes and to leave to a secondary role the dynamic characteristics. Meantime it became obvious, after works on advective dynamical analysis and after the theoretical research by I. A. Kibel, that a basic role should be assigned in the evolution of synoptic processes to dynamical characteristics, resulting from properties of thermobaric troposphere

fields. The advantage of this new viewpoint effected the theory of cyclo- and anticyclogenesis. The Norwegian school, by deviating from basic kinematic characteristics in their investigations of cyclogenesis, met difficulties and did not advance beyond a qualitative description of the process. As for anticyclogenesis, it was virtually left out of the discussion.

"The new point of view, based on the approximate hydrodynamical theory of baroclinnic atmosphere, in our opinion, handles these difficulties with more success and brings the investigation straight to the possibility of quantitative control".

Khromov cannot reconcile himself with this obvious and accomplished fact. This does not prevent him from being objective in the evaluation of the significance of the reviewed work, in which the frontal separations, as quoted in detail above, are investigated from basically new positions on a higher theoretical level.

The work by Pogosyan and Taborovskiy "Advective Dynamical Fundamentals of Frontological Analysis" should be a subject of careful study by synopticians as a work of great importance on the path of improvement of analysis and forecasting of atmospheric processes and weather.

- E N D -